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EXPERIMENTAL INVESTIGATIONS ON THE ANTIDOTAL AND REVIVIFYING PROPERTIES OF NITROUS OXIDE.

BY GEO. J. ZIEGLER, M.D.

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In the Journal of May 5th, I gave a resumé of the results of some experiments on dogs, which I had instituted for the purpose of securing more efficient means of preserving life, by resuscitating and sustaining the vital energies in cases of suspended animation from the asphyxia induced by the poisonous action of narcotizing agents, privation of air, &c., reserving the details for a future period, with the intention of prosecuting the investigation to a much greater length. A combination of circumstances, however, caused its suspension, and subsequently prevented its resumption. These details, therefore, I here present, with such remarks and deductions as seem appropriate.

To render these experiments more reliable, and the evidence therefrom as conclusive as possible, I adopted the following method of investigation, viz.—firstly, making a *test* experiment, by poisoning or otherwise asphyxiating the animal, and then employing the remedy for its recovery; and, secondly, a *comparative* experiment, which consisted in placing the same animal, by the same means, in a similar condition, though for fear of a fatal result, never permitting the asphyxia to become so great as in the former instance, and then trusting exclusively to the vis vitæ for a spontaneous recovery. The object of this comparative experiment was to obtain more correct data for the explanation of the phenomena presented, and to qualify the evidence afforded by the preceding, and, as far as possible, to exclude all sources of error, it was never instituted until after complete recovery from the effects of the latter, always allowing sufficient time for that purpose, generally from twenty-four to forty-eight hours.

Expt. I. *Carburetted Hydrogen.*—A dog was confined in a close vessel, and the common illuminating gas, taken directly from the pipes, introduced therein through a suitable orifice. After a brief period (the time was not noted) he was brought fully under its poisonous influence, and when taken out and thrown on the floor appeared to be quite dead; his body and limbs being somewhat extended, indicative of the rigor mortis; innervation, respiration and circulation having apparently been completely and permanently suspended. The nitrous oxide water was now injected into the bowels, and notwithstanding the profound asphyxiation, returning animation was speedily manifested by respiratory efforts, which rapidly increased to very strong and prolonged ones, similar to those produced by the direct introduction into the lungs, of the gaseous protoxide of nitrogen. This exalted respiratory action, however, subsided into the more tranquil and natural effort soon after the cessation of the injection. Also during and after the injection other evidences of the aroused vital excitability were presented by vomiting and purging, followed by the ordinary and more striking signs of returning animation. Consciousness was soon apparent, and manifested by the wagging of the tail in response to our calls, and some time before voluntary power had sufficiently and generally returned to permit the elevation and support of the body in an upright position. In less than twenty minutes the animal could, by coaxing, be induced to exert himself, though the locomotive efforts, at first, were very unstable, and not under full voluntary control. This power, however, rapidly returned, and before the expiration of one hour his energies were fully restored, and he was as actively and voluntarily exercising and eating as if he had not been subjected to any experiment whatever—not appearing to suffer in the least from the poisonous effects of the carburetted hydrogen. The quantity of the nitrous-oxide water used in this experiment, and introduced into the animal's body per anum, was about one quart.

Expt. II. *Comparative—Carburetted Hydrogen.*—About twenty-four hours subsequently, the same dog was again confined and subjected to the poisonous influence of this same agent. The asphyxia was not, however, suffered to become so profound as in the former instance. After, therefore, an incomplete and partial asphyxiation, he was removed and exposed to the sole revivifying power of the atmospheric air and sustaining influence of the vital energies, but without avail, as they were not sufficiently powerful to restore him at all, and death was of course the consequence.

Expt. III. *Chloroform.*—In this instance the asphyxia was produced by chloroform, using for that purpose about f ℥ j., by pouring it in the vessel previously used and confining the animal therein. This plan was adopted in consequence of the difficulty of controlling him sufficiently to exhibit it properly and to avoid its effects personally. The dog was soon reduced to a similar condition to that in the first experiment. Innervation was in complete abeyance, respiration entirely suspended, and although the heart's action was still perceptible, yet it was beating very feebly and frequently. The nitrous-oxide water was now injected into the bowels in small quantities at a time, until less than one quart was thus introduced. In a few moments after the commencement of the injections, indications of re-animation were apparent, differing, however, from those of the first, in the absence of the strong and forcible respiratory efforts previously exhibited (and which were probably dependent on the stronger injecting apparatus before used, and the consequent larger quantity and more rapid introduction of the surcharged fluid), they being in this instance more gradually and naturally increased.

They differed also in the existence of irregular or spasmodic movements, especially of the facial and masticatory muscles, with retraction of the lips, clashing of the teeth, &c. Complete recovery was, however, much more rapid, as in about eight (8) minutes the animal was capable of great voluntary effort, getting up and responding, by running, readily and actively to our calls, and in twelve or fifteen minutes was seemingly as lively and as well as before the experiment.

Expt. IV. *Comparative—Chloroform.*—The same dog was again, on the subsequent evening, subjected to the influence of this destructive agent. The asphyxia was not, however, so completely induced as in the preceding, the desire being not to sacrifice the life of the animal, but merely to obtain comparative results. In this instance, therefore, the respiration was not suspended, but only reduced, though considerably below the normal point, until it became quite slow and imperfect. The cardiac action was also less feeble and frequent, though there was complete insensibility and deprivation of motion. On being removed and exposed to the atmospheric air, recovery gradually supervened, accompanied with similar irregular muscular movements. The time required for a return to consciousness was, however, twice as great as that in the preceding, in which the surcharged liquid had been used. The restoration of physical power was also much more protracted, and the subsequent voluntary activity much less, with increased languor and lethargy and greater disposition to quietude and repose during the remainder of the evening.

Expt. V. *Carbonic Acid.*—In this case carbonic acid was used in the form of the fumes arising from the combustion of charcoal. The asphyxia was seemingly permitted to become too profound, as the usual attempts at resuscitation proved ineffectual and death resulted. This termination, however, appeared to result from the privation of the stimulus afforded by the nitrous oxide in consequence of the impaction of fæcal matter in the bowels, as the injected fluid almost wholly escaped therefrom during its attempted introduction. This impacted condition was also more strongly indicated from the impression of the fluid against a solid body.

Expt. VI. *Carbonic Acid.*—Another dog was similarly exposed to the noxious fumes of carbonic acid, which for greater convenience was obtained by the action of sulphuric acid on carbonate of lime; and to prevent the spasmodic closure of the glottis usually dependent on the attempted sudden inhalation of this gas, it was generated very slowly and in small quantities at a time. About one hour was thus consumed in bringing the animal fully under its influence. When taken out of the box, in which he had been confined, he was profoundly comatose; his body and limbs relaxed to flaccidity; respiration very slow, imperfect and labored; and a large quantity of frothy mucus and saliva was issuing from and filling his mouth. The asphyxia, though not so great as in the former test experiment, was sufficiently so to place life in a very precarious and highly dangerous condition, and excite just apprehensions of a speedily fatal termination. The injection of the nitrous-oxide water, in small quantities at a time, was now commenced, and continued rapidly until about two pints had been thus introduced, when complete recovery somewhat suddenly resulted, and in the short space of six (6) minutes of time, though animation had been gradually increasing from the commencement of the injections. The recovery was so perfect, and the activity and vigor so great, that he was immediately capable of vaulting and did vault out of a window, the sill of which was about twice as high as himself, being directed so to do from the manifest disposition to evacuate the bowels, the desire for which, immediately after recovery, was generally active in these experiments, though the passage was usually also involuntary during the production and existence of the asphyxia. Another frequent concomitant of re-animation was emesis. In less than twelve (12) minutes he had returned into the room in the same way (the distance from the ground to the window being still greater, and sufficient to require a somewhat unusual effort of a vigorous dog to successfully accomplish), and was eating as voraciously and heartily as if he had been deprived of food for a considerable period, and had never been, and especially so recently, in such an exceedingly critical condition.

Expt. VII. *Comparative—Carbonic Acid.*—The same dog was again, on the following day, for a similar period of time exposed to the poisonous vapor of this gas, and insensibility produced, with the usual accompaniments of frothy saliva from the mouth, &c. The asphyxia was not, however, so complete as in the former instance, in consequence of the exhaustion of the material for generation of the acid, and the indisposition to urge it to the same extent. On removal and exposure to the sole influence of the atmospheric air, recovery gradually ensued, but consciousness was not apparent till the expiration of eight (8) minutes, and no general voluntary effort was made till the lapse of about twelve (12) minutes, when he regained the upright position. Locomotion was, however, very languidly performed, nor even during the remainder of the evening did he exhibit any of that animation, activity and vigor immediately displayed by him on the previous evening when the nitrous-oxide water was administered. The character of the respiratory efforts during and after recovery was also strikingly dissimilar in the two experiments. In the preceding one, in which the antidotal agent had been employed, there was no perceptible increase in rapidity, but the gradually returning, gentle and natural action peculiar to a due arterialization of the blood, which had thus been already seemingly effected by the introduction of the protoxide of nitrogen. In the latter, however, the deficiency of the atmospheric stimulus and elements, and the desire and necessity for them, were energetically manifested by the active and forcible panting and rapid respiration which were continued for some time, until this deficiency was supplied and the equilibrium restored; thus proving most conclusively in the one case, that the sanguinary chemical equilibrium had been previously artificially re-induced; and, in the other, that it was so much in abeyance in consequence of the deleterious exposure, and the deprivation and absence of the atmospheric constituents, as to require a rapid acceleration of the function of respiration to restore it, by speedily supplying them. Another feature in the latter case was, that, instead of purging, recovery was attended with vomiting.

This was probably in consequence of the diversion and concentration of the vital energy on the bowels by the injections in the former, while, from the absence of such extraneous influences in the latter, and more in accordance with the usual course, the excitability was first re-established in the stomach.

Expt. VIII. *Hanging.*—Asphyxia was produced by strangulation from the suspension by the neck, taking care to avoid the organic lesion usually induced by the sudden forcible succussion of the neck from the falling of the body as in the ordinary process of hanging. This state of asphyxia unfortunately proceeded to a greater extent than was compatible with the existence and re-induction of animation, and death resulted. The time of suspension was about nine minutes.

Expt. IX. *Comparative—Hanging.*—The animal in this case was suspended by the neck in all eleven (11) minutes, though not continuously, the loss of the other dog causing more care to be exercised; hence when the cardiac action began to subside too rapidly, experience teaching us that it would fail very suddenly, he was relieved to ascertain the character of the respiratory effort. At the time above specified, the indications of insensibility (subsidence of the heart's action and diminished power of respiration) appearing to be sufficiently strong, he was liberated, but on the removal of the rope respiration was so immediate and perfect that I concluded not to administer the nitrous-oxide water, but to make this the comparative experiment. Consciousness returned very speedily (in about two (2) minutes), but no great or general voluntary effort was successful till the lapse of nine (9) minutes, when he succeeded in getting upon his feet and had recovered sufficient power to walk about, yet very languidly. This debility and lassitude continued during the evening, as he evinced no desire for voluntary locomotive effort, but remained quietly reposing in a recumbent position. Before the experiment, however, he was remarkably vivacious and playful. Recovery was attended with the usual rapid and forcible respiratory efforts to speedily renew the due proportion of the atmospheric elements and equalize the chemical condition of the blood; also for some time subsequently with strong sonorous respiratory sounds.

Expt. X. *Hanging.*—The same dog was again, on the succeeding evening, suspended by the neck for twenty-two (22) minutes in all, relieving him occasionally as before to determine whether sufficient vital energy remained to renew respiratory action. The temporary relief thus afforded does not, however, militate against the general result, but rather strengthens it, as this process more permanently and effectually debilitated the vis vitæ and proportionately destroyed the subsequent chances of recovery. By comparison these intervals were not more frequent than the former, and the whole time of suspension was twice as great. When the animal was finally liberated, innervation was abolished, respiration entirely suspended, and the heart's action imperceptible. In fact, these principal functions were so completely in abeyance as to simulate if not actually constitute that condition termed death. Under these unfavorable circumstances, the injection of the nitrous-oxide water was commenced, and after a moment of great doubt and uncertainty, to my surprise and gratification, the heart, as if from a sudden impulse, resumed its action by a somewhat decided though labored effort, the impression of which was obvious to the eye by the succussion and tremulous motion communicated to the thoracic parietes. Immediately following the cardiac action, respiration was re-established, but somewhat peculiarly, appearing to be dependent solely upon, and limited to, the movements of the diaphragm, as the thoracic parietes were fixed and quiescent while the abdominal region was in lively activity. The medulla oblongata and other nervous centres were, however, soon fully aroused and influenced as the respiratory efforts speedily became more general, vigorous and prolonged, similar to, though not so powerful as those exhibited in the test experiment with nitrous oxide and consequent recovery from carburetted hydrogen. Consciousness was not so immediately manifested as in the preceding comparative experiment, yet general voluntary power was much more rapidly, suddenly and perfectly acquired, as the animal was able to, and did recover the upright position, and made active locomotive efforts in five (5) minutes of time, while in the former he could not succeed in these efforts, and then but feebly, till the expiration of nine (9) minutes. Again, in a few minutes after recovery he ran to and actively vaulted into his box (an ordinary packing box), the sides of which were higher than himself; an effort he totally failed to accomplish after repeated languid attempts on the evening previous. Another striking difference from the former, was presented in the entire absence of that subsequent panting, rapid, forced and sonorous respiration, indicative of the necessity and instinctive desire for the atmospheric constituents; the respiration in this instance immediately subsiding into and resuming its natural character on the discontinuance of the injections. There was not immediately on recovery that vivacity and vigor which was so strongly exhibited in the previous experiment with, and artificial resuscitation from, the influence of chloroform, but a somewhat similar disposition to dulness as in the preceding. This was, however, of temporary duration, as in a brief period he was again quite active and lively, though not so much so as before the experiment.

Expt. XI. *Drowning.*—The dog was immersed in water for about three minutes, which unfortunately produced too decided asphyxiation and necessarily consequent death. There are good reasons for believing, however, that extraneous influences were in some measure operative in the production of this result, as the water was exceedingly cold, thus destroying more effectually excitability, and the absence of our usual assistance prevented the immediate and rapid supply of the nitrous-oxide water, hence it could not be exhibited sufficiently early and in proper quantities to insure success.

Expt. XII. *Drowning.*—As before stated, believing that the previous fatal result supervened to some extent in consequence of the deficiency and too late administration of the nitrous-oxide water, this experiment was repeated, and although the animal was immersed but about *one* minute, asphyxia was so complete as to render abortive the usual efforts at resuscitation. The only explanation which appears at all reasonable for the powerful depression and sudden fatality in these two experiments, is that the coldness of the liquid, in which the animal was immersed (the experiments being performed at that season of the year in which this liquid is at a very low temperature), connected with, though somewhat independent of, the asphyxia from the privation of air, produced such a sudden prostration or exhaustion of the vital energies from the shock (analogous to that from lightning, blow on epigastrium, &c.) on the peripheral extremities of the afferent nerves and thence communicated to the nervous centres, as to destroy so immediately and effectually all the vital excitability necessary to the continuance and restoration of life action, that no means or influence, however powerful, could arouse or induce sufficient energy to preserve the existence of the organic operations. Notwithstanding this explanation, it may be supposed, from the apparent deficiency of its influence in this form and in these cases, that the nitrous oxide is not applicable to such conditions; but it is believed that further experimentation will demonstrate its utility in this as well as in the other instances, and particularly if introduced in its gaseous form through the lungs, especially as in the process of drowning the exhaustion of the vital excitability is not usually so positive or immediate, its complete destruction or death generally being more gradual.

Expt. XIII. *Hydrocyanic Acid.*—In this case f ʒ ss. of the officinal prussic acid was poured down the dog's throat. Evidences of its effects were speedily presented by the failure of voluntary power, convulsive action and insensibility, during the inception of which vomiting was induced, and notwithstanding the supposed relief which this act of ejection would afford, the animal continued gradually sinking, and at the expiration of five minutes, conceiving that the effects were sufficiently strong, the injections of the nitrous-oxide water were commenced and continued till about two and a half or three pints (Oiiss. or iij.) were thus administered. After eleven (11) minutes from the introduction of the poison, and six (6) minutes from the commencement of the injections, consciousness was apparent, and about two or three minutes subsequently voluntary power began to return, but not fully until the expiration of about twenty-seven (27) minutes. This return was much more gradual than in the former test experiments, and accompanied with partial stupor and dulness, and inefficient or irregular muscular efforts and movements similar to those displayed under the influence of alcohol. In about three quarters of an hour, however, the cerebral and nervous centres and energies were more fully relieved and aroused, and shortly after, in less than an hour, the animal appeared to be as lively and active as before the experiment, eating freely and greedily.

Expt. XIV. *Comparative—Hydrocyanic Acid.*—On the following day the same quantity was administered to the same dog, with the production of similar effects, though not so rapidly or strongly manifested. In three minutes he vomited freely, and much more so than in the previous experiment, having had in his stomach, from the evidence thus presented, a very large quantity of ingesta, that ejected being twice or thrice as great as before. From this time improvement began, and in twelve (12) minutes there was speedy recovery.

Expt. XV. *Compt. Hydrocyanic Acid.*—Thinking that probably the quantity of ingesta and the perfect ejection of the poison prevented its effects from being more strongly experienced, the same experiment, twenty-four hours subsequently, was again repeated; but notwithstanding the vomited matter was much less in quantity, the poisonous effects were still less apparent than before, as the animal did not lose his consciousness at all, and very little of, and for a very brief period, his power of voluntary motion.

Expt. XVI. *The Same.*—Still conceiving that there might be some source of error in the deterioration of the acid, rather than that the dog had become so speedily accustomed to its influence, though suspecting this might be the true cause, nearly forty-eight hours were permitted to elapse before another attempt was made, when having procured some fresh acid, the same quantity was again administered, but without the slightest apparent effect of any kind. This proved most conclusively that the rapid depreciation of its effects was dependent on the increased power of the system to protect itself against this virulently poisonous agent. Dogs thus appear to become rapidly accustomed to the influence of this, and probably also all similar agents. In a practical point of view this is of some importance with respect to experiments upon them. This fact also renders the evidence afforded by the rapid recoveries in the test experiments stronger than before, as all the comparatives with these agents were secondary. Hence it is presumable that the differences were not so strongly marked as they would have otherwise been, particularly as in all of the latter the prospects for recovery, and that speedily, were much more favorable in consequence of the asphyxiation being much less than in those in which the nitrous oxide was administered.

Expt. XVII. *Aconite.*—In this case f ʒ ss. of the concentrated tincture of aconite was administered to a dog, which gradually exerted its influence on the system, especially on the stomach, vomiting being induced in nine minutes, with subsequently frequent efforts at such, attended with a somewhat copious discharge of white frothy liquid like saliva. In half an hour the animal was so fully under the poisonous influence as to be insensible and incapable of any movement, lying quietly on his side. Respiration soon ceased, but the heart was still beating, though very feebly and rapidly, and he was evidently *in articulo mortis.* At this time and in this condition of things, the injections of the nitrous-oxide water were commenced, and during the introduction of about one pint and a half (Oiss), respiration became established and the heart's action more decided and regular; and after the expiration of three (3) minues consciousness was apparent, and was manifested by the wagging of the tail in response to our calls, not having yet recovered the power of moving any other part of his body. This state of improvement continued for a few minutes, when suddenly he extended his limbs and body, and sank back again into his former insensible and dangerous condition; yet by the rapid injection of the nitrous-oxide water he was again brought out of this state and restored to consciousness, and so continued for a brief period, with every prospect of ultimate recovery. But he soon relapsed, and was again revived by the injections of the surcharged liquid. Such strong prospects were presented in favor of his complete and ultimate recovery, that strict attention to his condition was temporarily withdrawn (former experience having taught us that after the re-establishment of respiration and consciousness, perfect recovery was very speedy and certain), when he sunk so deeply and permanently that all efforts at resuscitation failed, and life was destroyed.

Several other experiments, with similar agents, as opium and empyreumatic oil of tobacco, were attempted, but in consequence of their ready rejection by emesis and the want of time to continue, we failed to get the animal sufficiently under the influence for the administration of the nitrous oxide; and, as before stated, circumstances at this stage caused the suspension of the investigation, and have since prevented its resumption and the previous presentation of the foregoing details. My intention is to resume these experiments, and also to review the preceding by the direct introduction into the circulation, through the lungs, of the gaseous protoxide of nitrogen; its influence in this form, thus exhibited, as is well known, being very powerful, and very rapidly manifested.

An objection to this mode of experimentation with water charged with this gas and thus introduced into the bowels, has been made under the erroneous view that a prominent source of fallacy or deception would arise from the stimulant effects of the water alone. To this I would answer, that the stimulant influence of this liquid is but transitory, and only stimulant when it is in momentary contact with vital surfaces, its prolonged application being powerfully sedative. Besides, the phenomena exhibited in recovery in the foregoing experiments, are in direct opposition to any such supposition, the character of the rëestablished respiration and the general recovery being entirely at variance with the hypothesis that they were excited or dependent upon the mere or exclusive stimulant influence of water or any other agent, the nitrous oxide acting both chemically and vitally. In fact, the difficulty in cases of poisoning from the more permanent narcotics, like aconite, opium, &c., is, that the antidotal influence must be continued for a proportional protracted period, and the use of the nitrous oxide in this form of combination with a liquid is on that account objectionable, the quantity of water being too great for prolonged administration, and not supplying a sufficiency of the active agent. The aqueous influence is, besides, directly in opposition to that of the agent desired, it being a powerful sedative when retained for any length of time in contact with the living surface, as it must be when thus introduced into the intestinal canal, the great proportion of the water introduced in these cases being retained, as the subsequent alvine evacuations are comparatively consistent and very much less in quantity than the liquid previously extracted. Again, when there is sufficient excitability still remaining in the system to be acted upon and promote recovery, it alone may destroy and prevent such by causing an undue concentration of the nervous and vital energy on the bowels, to the exclusion of the rest of the economy, to get rid of, or expel the extraneous agent therein existing; and in this way it may prove additionally injurious by robbing the great nervous centres, and destroying the general vital equilibrium sufficiently to retard, or entirely turn the scale against, and prevent the possibility of recovery.

Therefore this liquid, in this combination and in such cases, seems to be not only objectionable but positively injurious, except as a medium, its active ingredient being obliged to overcome both the sedative and depressive effects of this and the other influences. Hence in cases of poisoning from the permanent narcotics, it will be preferable to use the gaseous nitrous oxide, and introduce it directly through the lungs; while in those cases of suspended animation from the same class of agents having but a temporary influence, and in other analogous conditions from the privation of air, &c., the surcharged liquid would be applicable; and as shown in the preceding details, generally sufficiently powerful to induce and insure recovery notwithstanding the sedation and vital diversion produced by the liquid. In all cases, however, the former would be preferable; but as the nitrous oxide in its gaseous form is not so conveniently obtained, the latter might be employed either exclusively, or till the former could be procured.

In consideration of these facts, therefore, the following conclusions seem justifiable—viz.:

Firstly, That nitrous oxide or protoxide of nitrogen is a powerful and direct arterial, nervous and cerebral stimulant.

Secondly, That it exerts a direct chemical influence on the blood, by supplying the essential elements for the arterialization of that fluid, and to a certain extent by inducing that process, thus producing in it similar changes to those effected by the atmospheric air, as proved by the effect on, and character of the rëestablished respiration.

Thirdly, That it is in these various modes antidotal to the effects of certain narcotizing agents.

Fourthly, That where vital excitability is not completely destroyed, this remedy has the power of sustaining and increasing it rapidly, and sufficiently to preserve life in numerous instances in which it would otherwise be destroyed.

Fifthly, That it will rëestablish life action even after all the usual evidences of its existence have failed, such as innervation, respiration and circulation; provided, firstly, that the muscular contractility, or vis insita of the heart and other tissues is not lost; secondly, that the blood has not coagulated or deteriorated to such an extent as to be insusceptible of arterialization and revivification; thirdly, that there is no organic lesion of any vital part sufficient of itself to prevent recovery; and fourthly, that innervation is still susceptible of rëexcitation.

In conclusion, I will state that in my last paper on the therapeutic applications of this agent in the form of surcharged liquid more especially, I inadvertently omitted to mention a peculiarity in its physiological action, which, however, might be anticipated from its influence over the contiguous renal apparatus, viz., its stimulant effect on the generative organs, thus operating as an aphrodisiac. This effect, like its diuretic, is not, however, constant or universal; yet, nevertheless, its application may prove useful in atonic states of this apparatus. With respect to its favorable therapeutic influences and applications, therein detailed, I have no reason to change my views, further experience and reflection only confirming still more strongly all former observations and impressions.

Philadelphia, Nov. 5th, **1852.**

PROFESSIONAL REMINISCENCES OF FOREIGN TRAVEL.

[Concluded from page 369.]

There is diversity of opinion in regard to the methods adopted by Prof. Simpson in the treatment of uterine displacements, and diseases. By some, they are condemned, probably without trial—by others, warmly recommended, and fairly used. By some we are told that such diseases wear out by time, or the patient at length comes to tolerate what she has long suffered; and if the displacement remain, its symptoms may disappear, and this, with all sorts of treatment, or in the abandonment of treatment altogether. This spontaneous recovery, however, will not be always waited for. Treatment will be demanded, and treatment procured. The current amount of professional faith in itself, and its means, will settle, as it is now daily doing, whether regularly-bred physicians shall have the management of grave, and most distressing diseases, or whether they shall pass into the hands of others, whose faith exceeds their knowledge, and whose promises of cure may have an effect in the recoveries attributed to their ministries. In Prof. Simpson's methods in Europe there is confidence. He has for his friends in Scotland, Ireland, England, the support of some of the best men who have devoted themselves to the study and treatment of female complaints. He went within the year to Ireland with his friend, the celebrated Retzius, of Sweden, who passed with him part of the summer. He was welcomed to Dublin by the whole profession; and received its public hospitalities and honors, alike for his introduction of chloroform, and for his methods of treating uterine diseases. It is not difficult to explain how some methods may fail in the hands of those who are unacquainted with their use. They are mechanical—surgical, and skill can only come of experience. I was daily struck with the entire facility with which Dr. Simpson did his various operations. He has acquired skill by practice. Very, very little pain was ever complained of by the patients. He is very quiet, gentle in his manipulations, and is the object of the deepest interest and regard of those he serves. I have heard originality denied to him; and the proof was found in instruments longer in use than his own. For instance, the stem pessary of Boston was earlier than his, it is said. But this instrument has the stem on the *outside of the vagina*, and is only used to give points of attachment to straps to keep the instrument in place. In Prof. Simpson's instrument, the stem is passed *into the uterus*, reducing the dislocations which may have taken place in the organ itself, and restoring it to its true place in the pelvis if displaced; an agency which is not within the compass of any preceding pessary within my knowledge.

Ovarian Dropsy.—Among other surgical operations which I had an opportunity to witness in Edinburgh, was one for ovarian dropsy. This patient had presented herself at Professor Simpson's clinique, where I had seen her some days before. Mr. Goodsir did the operation. It was done to relieve her of great present distress—to discover the state of the abdominal viscera—to learn what was the condition of the sac as to adhesions—if there were more than one sac—if any induration existed—if the tumor were moveable, &c. &c. The simplicity of the arrangements for the operation interested me. At home, I had always seen the patient taken out of bed, and placed in a chair. A sheet is then carried round the abdomen, with an arrangement for drawing it, so as to compress the tumor as the water passed off. In Edinburgh, the patient was simply brought to the edge of the bed—a tub placed by its side. The trocar and canula are pushed into the sac, in a convenient place for the discharge of the fluid. The quantity was very large, and of an unusually dark color. After the ovary was emptied, and the canula had left the cyst which had contained the fluid, and so opened into the peritoneal cavity, a discharge of clear water at once took place, showing that along with the ovarian disease was ascites, an accident to the graver malady.

The patient was left very comfortable from the removal of her burden. I did not remain long enough in Edinburgh to learn the result of the case.

Chloroform.—This furnished a topic of much interest in Edinburgh. At breakfast, one morning, Prof. Simpson met me with these tidings from America. "Prof. C., the Boston Journal says that three more cases of death from chloroform have recently occurred in your country." I now asked again if he had met with any untoward results from its use in his own practice—with any thing which had produced in his mind the least doubt as to its entire safety. He said, no—he had not met with any accident from the use of chloroform. I knew Prof. S. introduced chloroform into the practice of midwifery, and of surgery, and that it might be said that such a relation to it would bias his opinions concerning it. I therefore looked for its use elsewhere, in the hands of others. The same answer came. Here were patients undergoing surgical operations; and chloroform, in its fullest use, in these, was found to be as safe as elsewhere. It was my privilege to see it used in midwifery, and it was as manageable as any other medicinal agent of real power, in any employment of it. There was one application of it, which seemed to me as much of a test-trial as well could be. This was its employment to produce insensibility in painful diseases, and as a means of diagnosis. I say a test or trial use, and for this reason. In the surgical cases which have been fatal under the use of chloroform, it has been employed to produce insensibility as a *preparation* for operations. It has not been when employed in the *midst of operations*, and to *continue* insensibility during the progress of the knife, that it has been fatal, but when used as a *preparation*, and especially in cases of slight surgical interest. It has never been fatal in midwifery, and the explanation is, that in these long-continued cases of suffering such a condition of nervous power is produced as to modify the action of chloroform. But in its use for diagnosis, such condition has probably been but partially produced, and still, in these, chloroform was perfectly safe. It was suggested that the occasional mortality after the use of chloroform may be owing to its impurity. I called, with Prof. Simpson, on Messrs. Duncan, Flockhart & Co., and examined their chloroform, and compared it with other specimens obtained from other chemists and druggists. The process was a very simple one, and though it showed a difference among kinds, it did not teach on what the difference depended. The process consisted in dropping a little chloroform on the back of the hand, and when the part was dry it was smelled to. No smell remained after the trial with Duncan & Co.'s. A disagreeable odor remained after another specimen was tested in the same way. Now though some impurity may exist in a specimen of chloroform, the question is not settled how far such would affect its medicinal use. I was assured that Duncan's chloroform was in extensive use in Edinburgh, and that no untoward results had marked its employment. Nor had such result, as far as I heard, followed the use of what seemed less pure chloroform. Messrs. Duncan & Co. told me that their weekly orders from London were between one and two hundred pounds of chloroform. Much more must be supplied by other manufacturers, for, as far as learned, this is the principal anæsthetic employed in Great Britain and on the Continent. Supposing impurity to have had no agency in producing the deaths which have followed the use of chloroform, we look to predisposition either of idiosyncrasy, or other, if such exist, and which may involve the same fatal results. The question which arises here is quite as difficult to answer as that which impurity of the article may suggest. In other words, we may know as little of one as the other, and the concurrence of both may be as little understood. We must then take the facts as they are, and each decide for himself what his course concerning anæsthetics shall be. I have brought with me a specimen of Edinburgh chloroform. One of our best druggists has examined this, and expressed himself so entirely pleased with it that he means to order one hundred pounds of it from the house of Duncan & Co., in Edinburgh.

It is not my purpose to discuss the question of the comparative advantages and dangers of chloroform and sulphuric ether. I gathered abroad that chloroform is much employed. In England, it has been seriously and strongly condemned by some, while many use it. In Scotland, I did not hear the word *ether* uttered in this connection. With us there is a divided opinion against it. One or two employ chloric ether—a solution of ether in alcohol—a tincture of chloroform, the produce of distillation, and of simple mixture with alcohol, being precisely the same. Others say that this has its sole power in the chloroform, as the brandy and water dram has its main claim in the brandy; and we have one case narrated, and which occurred here, in which death took place almost immediately after the inhalation of chloric ether. Many here use chloroform altogether, and without any more distrust of its perfectly safe powers than the faculty in Edinburgh have. By far the greater number use sulphuric ether, and are entirely satisfied with it. It is effectual, say they, and safe. For myself, I mostly use ether. There are cases in which this fails; and it may be when an important operation in midwifery is to be done. In such, I substitute chloroform, and have never had cause to regret its employment. Since I returned from abroad, I have had consultations in difficult labor, and in one half, I have found the attendant using chloroform—in the other, ether; the results were alike excellent in both. The forceps was used in these, in a state of perfect unconsciousness, and both mothers and children have done alike well. There is not on record a case of death after chloroform in midwifery practice, and I have no reason to believe that unrecorded cases have happened. In surgery, deaths have occurred. In these, death has followed the use of chloroform as a *preparation* for the operation. In midwifery, and medicine, its use is very much confined to advanced periods of the labor, or of the disease. In one complication of labor—convulsions—chloroform has acted as no other means known by me have. It has been more successfully used than has any or all other remedies. In my book on etherization in child-birth, p. 307, are ten cases of puerperal convulsions in which anæsthetics were used. Of these, six of the women did well, and three children were born alive. In another table, p. 330, are seven cases of puerperal convulsions, in which etherization was not used. Of these, six women died, and only one child was born alive. Now these are not picked cases. They occurred in the same year as did those in the other table in which etherization by chloroform was used. They are too many to be resolved into coincidences, and my latest observations of puerperal convulsions, furnish new evidence of the safety and importance of the use of chloroform in this disease. The latest trials of it have exceeded in interest the preceding ones. For in some of these, chloroform has been employed *at the very beginning* of the attack, and before any other means have been used, as bleeding, &c., and with the happiest results. In some of my latest cases, before I saw the patient, most faithful medication had been tried, and where the case seemed utterly hopeless. Chloroform has been curative. My friend, Dr. Crane, of East Boston, will recollect cases of this kind, apparently utterly hopeless, in which the immediate result of its use was permanent suspension of the fits, and rapid recovery. I am very glad of an opportunity to give this direct and strong statement of the whole good agency of chloroform in this fatal malady; and I believe I may recommend it to others on an amount of testimony on which they may rely, without going into the evidence. I will say, that chloroform in these cases has been far more, and immediately useful, than has sulphuric ether. I have seen the two tried, fairly tried, and am sure of what I say. In the convulsions of children, chloroform has also been useful. I have tried the old, the stereotyped plan—bleeding, bathing, vomiting, purging—faithfully tried, and have known the fits steadily to go on. Under advice or consultation this treatment has been continued, and in vain. I have known fully the beneficial effects of chloroform in these cases. I have been informed of cases in which chloroform has been used in the *first instance* in fits of children, and of its good effects in such exclusive use of it. In the progress of a case of diseased brain, in a child three or four years old, and in which effusion was believed to exist, convulsion became a most distressing complication. The mother, advanced in pregnancy—looking daily for labor, asked if some means could not be used to diminish such constant and terrible expressions of suffering. *Sulphuric ether* was tried in this case. The convulsions were lessened in frequency, and soon ceased. This child recovered. In the use of such an agent as chloroform in such cases, the greatest care should be observed. Sponge should never be used for applying it. *I never saw sponge used abroad.* A bit of cloth, folded on a handkerchief as taken from the drawer, is always used. Very little is poured, or, better, *dropped* upon its centre, and the cloth is always so held as to allow a due mixture of atmospheric air with the chloroform vapor. As soon as its effects are perceived, the handkerchief is removed. Unless such, and all other needed cautions are used, it is not to be wondered at that untoward, even fatal results should be produced.

I have spoken of the use of chloroform in Europe, as I saw it used and heard it spoken of. I have alluded to the general confidence in its safety, and of the questions which now and then arise there concerning this important point in the employment of this active medicinal agent. While writing I have received Rev. Dr. Parker's Report of his Hospital in Canton, China, for 1850 and 1851, and this pamphlet from a distant land has a word concerning chloroform. In 17 surgical operations—9 for stone, and 8 for the removal of tumors, some of enormous size—chloroform was used, producing entire insensibility, and without any untoward occurrence. No such occurrence is alluded to in the pamphlet, which considering its source, is the best evidence that such has not been met with in Canton. I state the grave character of the operations, the length of time required, and but for chloroform, the exquisite suffering. I refer to this especially, because most of the mortality after its use elsewhere has occurred when it has been employed as a *preparation* for the most trifling operations in surgery, when the general health was good, or had not been impaired, nor the nervous impressibility lessened by preceding disease and suffering. I mention this Report, not merely to allude to the important professional facts it contains, but to express my grateful sense of Dr. Parker's kindness in sending it to me. Rev. Dr. Parker is at the head of the Canton Hospital, which in 1850 received 4712 patients, and in 1851, 4103, about one half of which were for diseases of the eye. Dr. Parker treats all cases without fee, whether in or out of the Hospital, and whether poor or rich, and with extraordinary success. He is training native pupils for the same service, and this, too, with encouraging success. In this way he has secured an interest in his missionary labors which is very striking. The evidence is in the letters of thanks of those he has cured, and which are in this Report. You see how these converts love him, with what deep reverence they allude to the sacred writings which he is unfolding and explaining to them, and you cannot withhold from him your admiration, your interest in the success of his extraordinary labors. The quantity of chloroform used by Dr. Parker, was small, sometimes half a drachm only, at others two or three drachms.

I have stated above what I saw done with chloroform in Edinburgh, and what is recorded of it in Canton, how common its use, and how safe has been its employment in those cities. But it is as well known that death elsewhere has followed [illegible] inhalation, in too many cases (between thirty and forty), and too soon after its exhibition to leave any doubt that it was the cause of those deaths. This fact in the history of chloroform has induced many medical men to abandon its use entirely in any of its forms, and has confined its employment by others to few and rare cases.

Sulphuric Ether.—The anæsthetic effects of this substance were discovered and demonstrated in America, in Boston, by a wide and universally safe use. With many it has recently replaced chloroform, and to the entire satisfaction of the best medical observers. I have above alluded to my own use of chloroform. I have published between one and two hundred cases of its safe inhalation in labor. I say the same thing again; and add, too, that among the deaths after its use in Europe and America, not a single one has occurred in obstetric practice. It is a highly interesting fact that chloroform has been most frequently followed by death in cases of minor surgery, and as a *preparation* for trifling as well as important operations. But of this I have already spoken.

Now in this view of our subject, may I not ask why we do not, and why should we not, rather confine ourselves to the use of *sulphuric ether*, which is perfectly safe—which I have seen used in wasteful profusion, and always without the least harm, than to employ chloroform, which has been followed by the gravest results, the causes of which are wrapt in such impenetrable mystery as to make it impossible for us to make any calculations concerning them? I have said that cases may arise in which sulphuric ether may fail, and in which chloroform has been successfully used. In such it may be used still.

MANCHESTER, ENGLAND. — My latest days abroad were passed in Lancashire, the great manufacturing capital of which is Manchester. It contains towards half a million of people, and the laboring population of the two millions of the shire are daily and hourly working for Manchester. What the amount of this labor is, you learn from the enormous warehouses—the mills, and the crowded Exchange which can accommodate three thousand, but which must be enlarged to meet the daily increasing demand. This noble city was to me a place of deep interest. What had been done and what was doing, for so many men and so many women congregated for work—yes, to devote their lives to useful labor—to labor, the products of which were to make an important part of that supply which the world's want demands? I answer that every arrangement has been made or is in progress for the intellectual and moral culture and the physical health and comfort of this most important order in any and all communities. I have no room here for detail. But the proof was everywhere—in libraries, schools, reading-rooms, parks, walks, model houses, washing establishments, hospitals, &c. &c. I was delighted to wander about such a city, and to learn what were existing arrangements for most important interests. I do not say that the whole demand is supplied; but I can say that hearts, and minds, and hands are at work here to do what may be done to meet so wide a demand.

Professor Simpson kindly gave me letters to Dr. Clay, and to Dr. Reid, of Manchester; and a correspondence of many years, and the interchange of professional and literary works, had for a long time made me acquainted with Mr. Surgeon Roberton, one of the ablest medical writers of the day. Professional engagements out of the city prevented my seeing Dr. Reid, and so my time came to be divided between Dr. Clay and Mr. Roberton. The hospitality of both those gentlemen I shall not forget. With Mr. Roberton I had my home.

With Dr. Clay I passed many hours in his carriage, in his study, at his table. He showed me some of the public works to which I have alluded, especially those devoted to the highest culture of the operative. He has been a laborer, and a successful one, for his profession. "The British Record of Obstetric Medicine, Surgery, and Diseases of Women and Children, &c. &c., to which is annexed a Library of rare Obstetrical, Medical, and Surgical Monographs, &c." is among the works which he has contributed to medical literature. It was continued two years, and is as honorable to the author's industry, as it is useful to the profession. It did not receive that patronage which it richly deserved, and which was necessary to its continuance, and ceased with the completion of its second volume. I regard this as one of the most important additions to my library. Dr. Clay's library is rich in the rare and valuable in medicine. He showed me his treasures in this way, and most curious are they. He has copies of the earliest works in medicine, especially on midwifery, and in a variety of editions and languages—the history of medicine, in permanent, and trustworthy records. Dr. Clay will have a lasting and honored memory in his Operations for the Extirpation of Diseased Ovary by the large Incision, between Sept. 12, 1842, and Sept. 17, 1852, the day I left Manchester. The following is written on a fly-leaf of a copy of Dr. C.'s volume, containing his operations, which he gave me, and which was published in 1842.

"*Manchester*, *September* 17*th*, 1852.—Since the publication of the first forty cases (annexed), I have operated fifteen times, four of which have died, and eleven recovered, making a total of fifty cases, of which eighteen died. CHARLES CLAY, M.D."

The comparative success of Dr. Clay's operations, I am not able to estimate, as I have not at hand the operations of others with which to make the comparison. In his large experience in this way, he has had under his care the great variety of forms under which chronic diseases of the ovaries show themselves. He has operated on the least promising; and when his diagnosis, made with all care, has been amended, or set aside by the revelations of the operation, he has nevertheless gone steadily on, except in one remarkable case, in which it was clear that such was the extent and whole size of the base of the tumor, as to make it sure that to have cut through it must have produced fatal hemorrhage. Dr. Clay has operated against a weight of professional opinion, heavy enough to have discouraged any man. Some may think that it would have been better not to have referred to this in his report of cases. But is not the general value of his diagnosis and that of his operations always increased, by that opinion which asserted beforehand their dangers, and afterwards that they were unnecessary? I have now Dr. Clay's work on this subject, and cheerfully express my admiration at what he attempted, and my exceeding pleasure at his success.

My last day abroad was spent in Liverpool. I had little time for explorations here; and contented myself with a drive with a friend over this great city—visiting its docks, and various other arrangements, for so much of the commerce of the world; and next morning, in the good Steamer Canada, with excellent Captain Laing for master—and with one hundred and forty-one other passengers, and with one hundred and seven steamers in company, begun my return voyage home.

I remain very truly yours, W. CHANNING.

Boston, Oct. 25th, 1852.

M. RICORD'S LETTERS UPON SYPHILIS.

Addressed to the Editor of L'Union Medicale—Translated from the French by D. D. SLADE, M.D. Boston, and communicated for the Boston Medical and Surgical Journal.

TWELFTH LETTER.

MY DEAR FRIEND,—Does there exist any real difference between the natural and the artificial contagion? This is the subject of our conference.

The observation and rigorous analysis of facts demonstrate to those who do not suffer themselves to be led away either by prejudice, or by preconceived ideas, that the contagion of syphilis, under whatever circumstance it may operate, is reduced in the final analysis to a process of inoculation more or less analogous to the process by the lancet. The lancet, in fact, inoculates the accident (the chancre) which by the confession of all is the most fatally contagious. It is by this accident, by the chancre, according to observations well made and *collected in the proper time*, that syphilis commences.

Laying aside artificial inoculation, the chancre is seen to develope itself everywhere upon the surface of the body without choice of seat, and upon all the external or internal integument, which is accessible, and by consequence, without there being need either for the parts which are infected, or for those which furnish the infecting matter, of special functions or of any particular physiological condition. Other conditions are necessary for contagion.

Examine with care all the parts which are affected, you will find that it is those which present the most favorable conditions for mechanical lesions, for scratches, for lacerations, and for solutions of continuity of every kind; you will find, also, that it is there where voluminous and numerous follicles exist into which the virulent matter can introduce itself, that the accident is by preference developed.

Is it not true that in the male it is more particularly the border of the prepuce, especially when there is a phymosis more or less pronounced, the neighborhood of the frænum, the adherent points of the semi-mucous surface of the gland and of the prepuce, points which not having the suppleness of other regions are more easily torn, that by preference become infected by the contagion; in the female the fourchette, the points of insertion of the nymphæ, the carunculæ myrtiformes, are the parts which most easily take on the contagion. In the other regions, is it not true that it is when excoriations exist that the contagion is established? Thus, an excoriation upon the finger is often the door where syphilis can enter. But the presence of an excoriation is absolutely indispensable. If it was otherwise, should I ever go out of the hospital without having a chancre at the end of each of my ten fingers? The chancre often appears upon the lips, but the lips are almost always cracked;

pleasure excites the smile, and smiling extends and dilates the lips. The nipples of nurses are often the seat of chancre, but these parts are ordinarily cracked and torn. The chancre seats itself everywhere where there has been a cicatrice, but there also there is a loss of suppleness, and consequently cracks and lacerations are easy.

In all this, my friend, you see nothing which is, as they say, physiogical, which exacts special vital conditions, a particular state of organism and the exercise of any function whatsoever. All this, for you, as for myself, is reduced to a traumatic and mechanical phenomenon.

Practice, that criterion of all doctrines, justifies, alas too often, my doctrine. Nothing is more common than to see the physiological act of generation rest indemnified from every unhappy consequence, while other acts which have nothing in them physiological, draw after them painful results. The genital organs, the seat so special of syphilitic affections, do not always take the infection from genital organs. It is not always the genital act properly called, which becomes the infecting cause. Coitus does not become an infecting act unless certain material circumstances come into play. Among the innumerable examples which I could cite for the support of my opinion, I ask permission to cite to you two, which have struck me more, inasmuch as they presented themselves to me suddenly upon the same day. There is no physician who does not know that there are some singular days, when curious facts arrive as if in series.

A gentleman brought me one day his mistress, whom he had infected, and in a manner which much astonished him. He had upon the penis a primary ulcer at the period of specific progress. He had had normal intercourse with his mistress, and in the same night intercourse more to be blamed, *à prepostera venere.* The lawful intercourse had been much more frequent than the other. The woman presented absolutely nothing suspicious upon the genital organs, but she had a chancre on the anus. What did this mean? That the physiological and natural passages had yielded without laceration, and had escaped contagion, while the abnormal passages, more resisting, were torn and became infected.

Here is another couple. Here, again, is a contest between a physiological act, and a prelude which does not belong to the human species, a prelude which is not at least placed among the genital functions of man. A gentleman surprised at seeing a suspicious bud pushing forth upon one of his lips (bud without a flower, as Jean Lemaire would have called it), without any disease of the genital organs, comes to ask me to examine the woman with whom he had had intercourse. I found upon this woman a chancre at the specific period, situated in the neighborhood of the meatus urinarius. This gentleman had had rather frequent sexual intercourse with this woman during the same night, during which he had gone astray so far as to sadly expose his lips. It is necessary to add that this gentleman was very subject to chapped lips, and that all this passed in winter.

These facts, which I could multiply, prove that the physiological conditions of the genital act go for nothing in the contagion of syphilis. Thus, the doctrine of physiologism finishes upon this point by falling to the ground. Be well convinced, that in spite of the most intimate contact, of the most complete fusion, and of the orgasm the most voluptuous, with an entire skin and an irreproachable mucous surface, one can escape safe and sound from the most exposed intercourse. Be assured, on the contrary, that a portion of skin torn, a mucous surface chafed, will render the slightest intercourse dangerous, and we physicians have a thousand precautions to take in this respect, and certainly our examinations are strict. We know, however, that the medical corps has furnished victims to the martyrology of syphilis, and that it was in the beneficent exercise of our art that the unfortunate Hourmann, and Delavacherie of Liége, found a death tediously frightful.

After what I have just told you, what can you think of the pretended physiological inoculation of my colleague M. Vidal, as regards blennorrhagia? You know when and how this latter is really inoculated by the lancet. It is then, and only then, when it proceeds from a chancre, and it is the rarest case, as M. Vidal agrees with me. But in other conditions in which blennorrhagia is produced, is there, physiologically and pathologically speaking, anything which resembles the contagion of chancre? Do we even always know, as I have said, till tried, if the blennorrhagia is always due to a veritable contagion? And yet this condition of contagion has been considered as a proof of virulence, as a sort of physiological inoculation, which the lancet cannot produce. Hear M. Baumès—it would seem that the successive contagions of blennorrhagia were his means of diagnosis, without telling us, nevertheless, how many times blennorrhagia ought to be produced in order to be virulent. Thus one takes a blennorrhagia, he gives it to another, where commences the virulence? M. Baumès does not say. Suppose that a woman is suspected of having contracted a discharge from a suspicious man—if we should wish to assure ourselves upon the nature of the discharge of this woman, it would be necessary to hold an inquest, to run after the different sources of the blennorrhagia of the man, and to pursue it, going back even to the gonorrhœal flux of the Bible. Yes, but we should not have made one step in this inquiry, without finding ourselves in the presence of that most common difficulty, of two individuals having had commerce with the same woman, the one will have contracted a blennorrhagia, and the other not. For one, we should conclude upon the benignity of the blennorrhagia, and for the other upon its virulence. All this is not serious.

Facts and observation, then, do not indicate any difference, my friend, between the inoculation called physiological and the artificial. Let us now invoke analogy.

In every malady incontestably contagious we find that the traumatic conditions dominate, and that under ordinary circumstances art can repeat what nature does. Thus, the vaccine inoculated does not differ from ordinary vaccine. The variola inoculated does not differ from the spontaneous variola. Thus, with the glanders, the farcy, hydrophobia, malignant pustule, and hospital gangrene. This argument from analogy appears to me of incontestable value. Why should the syphilitic virus alone escape from the common rule?

But the chancre, it has been said, is not the only contagious syphilitic accident. There are some secondary syphilitic accidents for which the lancet has not yet discovered the *contagium*. Science, in fact, contains a great number of observations which appear conclusive for a very large number of physicians, and which leave some doubt in the minds of many others. The numerous tubercles, or condylomata, are considered by a very large number of writers upon syphilis as contagious, and by consequence can be transmitted.

When I have studied this accident by means of inoculation, considering well all the circumstances which could prevent error, the experiments have always been negative. However, other observers have obtained contrary results. I can only answer for this exception by stating the result of my own experience.

I inoculated with the pus of numerous tubercles coming from the neighborhood of the vulva of a young girl of Versailles, who entertained habitual intercourse with the garrison of the place, and I obtained a positive result. Much astonished, I examined with more care the surfaces from which I had taken the pus, and it was then easy for me to recognize that among the numerous tubercles, there existed a chancre still at the period of specific progress. Then, some new inoculations being made comparatively with the pus taken upon this ulceration, and with the matter of the mucous tubercles at a distance, the pus of the chancre gave the characteristic pustule, and the muco-purulent secretion of the mucous tubercles remained without result. This experiment appeared to me decisive.

In the observations which have been cited of mucous tubercles which have communicated syphilitic accidents—the period which has passed between the time of observing the patient and the infecting coitus has not been taken into account. It is always three weeks, a month, two months or even more after the contagion, that the patients present themselves to the physician, so that not only the real form of the commencement is wanting, but still it is impossible to determine the true nature of the accident which has been the source of the contagion. Some individuals forget, and others do not know, that by a succession of changes easy to observe, where one takes the pains, the primitive accident (the chancre) passes *in situ* from the state of an *organ* of virulence to the conditions of a secondary accident, not furnishing any more specific pus. Where are the observations of persons seen with mucous tubercles, who have transmitted the disease to another person which could be observed the second or third day after the infecting coitus, and in whom the disease has commenced as we see it commence after the contagion from a chancre? Does the disease in this case commence with the chancre, or with the mucous tubercle? There is not one single incontestable fact which can answer this question. The facts, however, do not fail as regards mucous tubercles. As to myself, I possess very numerous observations of well-characterized mucous tubercles upon men and women, which prove that the patients thus affected could indulge in frequent sexual intercourse without communicating anything. Among all these facts here is one, my friend, which will remain deeply impressed in the minds of my readers, as it has in my own.

A gentleman whom I had attended for a chancre two years before, was about to marry. Before his marriage he came to see me again, in order to submit himself to a rigorous examination. I found him in the best state of health; he could be married without any scruples. However, this gentleman, a very strict man, exacted of me another examination the very evening of his marriage. I still found him perfectly exempt from every accident, and I delivered to him my bill of health as clean as possible. One month after, he sent for me. My dear doctor, he said, my wife has some large pimples upon her which trouble her very much. See what it can be. Before passing into the chamber of the wife, I proceeded to a new examination of the husband. I found him in as healthy a state as the day of his nuptials.

But it was not the same with his wife. I found some confluent and well-developed mucous tubercles, so as to assure me that the point of departure of the accidents was anterior to the marriage.

Convinced that the husband had nothing to do with this sad affair, and that he could not communicate a disease he did not have, I said to the wife in a firm and decided tone—Madam, you are diseased, and it is not your husband that has rendered you so. If I become your confidant, I become also your accomplice; in the contrary case, I shall remain the physician of your husband. I was not long in obtaining a painful confession, which gave me the key to this unhappy enigma.

I recount to you this fact because it contains this which is interesting, viz., that since marriage the husband had not passed two days without having repeated intercourse with his wife, and notwithstanding, he had absolutely no disease.

I have not finished with the mucous tubercles; permit me to return to them in my next letter. Yours, &c. Ricord.

EXAMINATIONS OF DRUGS, MEDICINES AND CHEMICALS, &c.

To the Editor of the Boston Medical and Surgical Journal.

A book with the above title was published in the early part of this year, which I have had in possession for some time, but want of leisure or something else has prevented my examining, though I have seen several notices of it. It was got up by one of the government drug inspectors, favorably known to the profession as the translator of Stockhardt. To city practitioners, whose neighboring drug-stores will enable them to prescribe articles with the probability of being well served, the book may not be of great advantage. There are two classes, however, who cannot fail highly to appreciate its merits; the retail druggists, and those out of town physicians who are still obliged to carry the pocket-case and saddle-bag. It is not alone from the deception of the foreign manufacturer, that the people need protection. There is a great chance for adulteration by home manufacturers, and the name of a supposed responsible house is not always a guarantee that a medicine is what it purports. This personal experience has shown to me. In looking over Dr. Pearce's book, I had marked a number of passages, that might be quoted and read more than once with advantage; such articles as refer to our most commonly used drugs; but before concluding the book, they became so numerous, that in doing so, I should quote more than half the book. The rules given are easily followed, and easily understood, even by those not very much versed in the science of chemistry. A practitioner who has the common apparatus for analyzing urine, milk and other secretions, has about all that is needed for satisfying himself of the state of purity of such medicines as he wishes to buy. One great advantage, that the habit of examining would produce, would be the check upon our home dealers, as every physician would become a drug inspector.

I believe that a notice of this kind may induce some more of our profession to follow this subject for themselves; if they begin to do so, they will find the occupation so agreeable, that they will not easily be persuaded to relinquish it. EXAMINER.

SOUTERN TYPHOID FEVER, QUININE, DR. FENNER, TYPHUS, &c.

To the Editor of the Boston Medical and Surgical Journal.

SIR,—I noticed in your issue of the 10th inst. an editorial with reference to the arrest of typhoid fever by the use of quinine, under the abortive plan of Dr. Fenner. With all due deference to Dr. Fenner, as a gentleman of veracity and practical acumen, permit me to enter my protest against the validity, if not the utter impracticability, of the remedy, in easily cutting short typhoid disease of the South. In our estimation, Dr. Fenner has christened his remedy rightfully, "*abortive*." We venture to assert, that no real unadulterated case of typhoid fever can be arrested by such medications. We observe in the South, a form of typhoid fever characterized by *intermissions*, in which the quinine goes most admirably, and will surely arrest; and it is upon these cases Dr. Fenner must have stumbled in his peregrinations in Mississippi. We have devoted considerable attention to the investigation of typhoid fever, we have done a heavy practice in that line, we have seen it in every imaginable phase, we have adopted all practicable and to us judicious plans of management, from sage tea and mercury down to quinine and pepper, and it is our deliberate opinion that *no plan of treatment, yet suggested, will cut short easily that peculiar affection.* This opinion is formed upon an enlarged experience, backed by numerous post-mortem results, and comfirmed by the opinion of about seven-eighths of the southern physicians, particularly that class of them who do about two-thirds or more of southern practice, and see more cases in one season than many of our city medical men do in several—*the country practitioners.* We think we hazard nothing in saying, that in all the plans of treatment suggested in typhoid fever, there is no one upon which the southern profession are so generally united in opposition, as the quinine. It is a common opinion in some localities, and we have no question of its force, *that the extrava-*

gant use of quinine has induced the affection in this climate to some extent. The position is plausible, and we conceive susceptible of demonstrable maintenance. That there are various shades of the disease among us, no one denies, who has seen much of it; but upon the applicability of the quinine treatment, save in the form we mention, we are almost a unit at the south. There are some, it is true, who deny the existence of the disease at all, but the number is small, and getting less daily. One man disavows its existence, and says it is "*ship fever*," and consequently we have none of it, for we have no "*ship fever*" in this inland country, nor will we ever have, we presume; but the idea of the non-existence of typhoid fever, because it is not identical with "*ship fever*," is the sheerest fallacy.

The existence of pure typhus south in the country, is a matter of great doubt with a large majority of medical men. We have never seen a case, and although our census report records cases of real typhus, we should question very much the truth of the report as to the existence of any such affection in our rural districts, and for reasons which are impregnable to our mind.

That real typhoid fever exists here, is clear and unquestionable; the specimens before us now in our office, would convince the most radical sceptic; and we believe the differences of development in the glands of Peyer and Brunner in the disease south, are quite as clear and conspicuous, over the northern forms of the disease, as the existence of the disease itself. As to its therapeutic management, we can only say it requires better care and less medicine, than any febrile disease we ever contended with, and we would be glad to see the man who could *cure or arrest* it. SOUTHERNER.

SUDDEN CHANGE IN COLOR OF THE HAIR.

To the Editor of the Boston Medical and Surgical Journal.

DEAR SIR,—Since I reported the case of Dr. Capen, he has directed my attention to one fact which I omitted to mention. In the progress of recovery, his hair, which before was very gray, and in places almost white, became quite dark, and even black in some spots, mingled with a few gray hairs.

Dr. Capen referred me to Dr. Wilson on diseases of the skin, page 377. "Dr. Bruley, a physician of Fontainbleau, communicated to the society of medicine at Paris, in the year 1798, the history of a woman sixty years of age, whose hair, naturally white and transparent as glass, became jet black four days before her death. She died of phthisis. Some of this hair was transmitted to the society, and was found to be quite black, with a few white hairs interspersed. On examination after death, Dr. Bruley found the bulbs of the black hairs of an immense size, and gorged with dark pigment. The roots of the white hairs which remained, were dried up, and two-thirds smaller in size than those of the black hair." In remarking upon this case, Dr. Bruley observes—"It is certain that disease may give rise to a change in a short period, that, according to Haller, requires a long period to accomplish naturally." Second American edition—1847.

The case of Dr. Capen resembles in many particulars that reported by Wilson. Yet the Doctor has not yet condescended to allow us the privilege of a *post-mortem*, and we trust he will continue obstinate in this particular many years to come.

Dr. Copland, vol. 9, page 162, remarks that, "gray hair has in various instances been changed to black." It may, by a faithful application, morning and evening for three weeks, of the following formula, be restored to its natural color. In some cases, however, it requires a somewhat longer time. R. Sup. acet. plumb., ʒ ij; sulph. lac., ʒ j; aq. rosæ, ℥ iv. M. When applied it should be well rubbed in with a small sponge. The effect of its application is, first, to produce a vigorous action in the skin, attended by a slight itching. The hair at the first change becomes of a pale yellow, and from that gradually comes back to its natural color. If this application should be applied to alopecia partialis at its commencement, I believe in nine cases out of ten it would fully restore the hair, and it has as yet succeeded in every instance in restoring the hair to its normal state, by exciting the bulbs to a natural and healthy action.

Alanson Abbe, M.D.

MEDICA SACRA—EMBALMING OF THE DEAD.

BY DR. THEOPH. RUBINSOHN.

[Communicated for the Boston Medical and Surgical Journal.]

The embalming of the dead may be traced back to the most ancient times of historic recollections. The end which was thought to be obtained by it, was the preservation of the bodily form of the corpse as long and as completely as possible, and to prevent it from putrefaction. Embalming was not only the custom among the Egyptians, but also among other nations of western Asia and northern Africa. The Grecian historians inform us that the Æthiopians, Persians and the Scythians understood the art of preventing decomposition of the dead [Herodotus, L. iii.. c. 24; Strabo, L. xvi.; Josephus, Ant. Jud. L. xiv., c. 7). This custom we find also prevalent among the ancient Hebrews. But before we proceed to treat on this subject more fully, we make here the remark, that the statement in the New Testament (John xix., 39, 40) that "Nicodemus came and brought a mixture of myrrh and aloes, about a hundred pound weight, and took the body of Jesus, and wound it in linen clothes with the spices," does not refer to an actual embalming of the body of Jesus, but it was then the custom among the Jews, in imitation of the Romans, to anoint the dead with fragrant ointments, that the injurious odor of decomposed bodies should be removed.

The following passages from the Old Testament refer to actual embalmings. "And Joseph commanded his servants, the physicians, to embalm his father; and the physicians embalmed Israel. And forty days were fulfilled for him; for so are fulfilled the days of those which are embalmed."—(Genesis l., 2. 3.) "And Joseph died, being an hundred and ten years old; and they embalmed him, and he was put in a coffin in Egypt."—(Gen. l., 26.) The sacred writings do not describe the manner in which the physicians embalmed the dead, nor the instruments that were used at this operation; it is only stated that the operation was continued during forty days. It is, however, very probable that the operation was performed after the method prevalent among the Egyptians, and it might be perhaps of interest to the readers of this Journal to learn the results of our inquiries on this subject.

Manifold are the reasons which the antiquarians ascribe to the custom of embalming, which art can be traced back to the early history of Egypt. That it required skill and experience, can be seen from the mummies which are in the British Museum in London, and in other parts of Europe. The name of the embalmed bodies is "Gabbara," i. e., the sacred preserved, or "mumia." The first name is Egyptian, the second Persian. Augustin in his sermons (120, c. 12) says—"Ægyptii credunt resurrectionem mortuorum; morem enim habent siccare corpore et quasi ænea reddere; Gabbaras ea vocant." In the Œdipus Ægyptiacus by Kircher, are these words: "Mumia vox Persica est, et idem notat quod exiccatum cadaver, certa ratione conditum, corruptionis expers." A French writer is of the opinion, that the Egyptian priests have introduced the art of embalming the dead with the benevolent purpose that the health of the living might not be impaired. The annual outpourings of the Nile, through which the ground of the country is made soft and loose to a great depth, produce the quick decomposition of the dead bodies, and cause pestilence. To prevent this calamity, the art of embalming human and animal bodies has been invented; but in the course of time, the priests concealed its real cause, and connected it with their religion, which served as a political means to make the people obedient to the civil laws. To support this opinion, it is said that up to this day the pestilence in the Orient breaks out first in Egypt. The pestilence, it is further said, in the Orient, made its first appearance in the sixth century of the Egyptian era, when the embalming of the dead was not more common, and that it never prevailed in Upper Egypt where the Nile does not pass its natural boundaries. But these reasons are barely hypothetical. For if the priests had feared the consequences to the health of the people, they might have suggested the burial of the dead in the adjacent desert, where they could have been dry in a very short time. The art of embalming needs not to be explained from reasons so remote and complicated, as there may be advanced others that are more natural and more probable.

The people in Upper Egypt might have had frequent opportunities of observing the phenomenon, that persons who died in the desert, or were overtaken by the whirlwinds of glowing sand and buried in it, were not decomposed, and having become dry in the heat their forms and appearance remained perfect and not at all destroyed. When the counter winds carried off the sand from the bodies, they were then recognized by their relatives and friends, and taken and buried in their respective homes. Such bodies are known by the name of "natural mummies." They were dried by the extraordinary heat, so that nothing remained of the moisture in the animal body, which causes decomposition. Herodotus relates that Cambyses sent fifty men to destroy the oracle of Jupiter, who were buried in the sands of the desert, and were afterwards found by the Ammonians in their perfect form. The dry mummies of the desert have a shining brown appearance; the skin is dry as a parchment, and cleaves to the bones, which are perfectly dry ; every particle of moisture and fat has disappeared, and the whole body frequently weighs not more than sixteen ounces. But there are also instances of natural mummies, which were not buried in the sands of the desert.

As many arts have been invented through a close observation of natural phenomena, the art of embalming the dead owes probably its existence in Egypt to that natural phenomenon, that we have now been describing. The natural mummification might have excited the observer to experiment on some of his relatives, whose form and appearance was dear to him, to preserve his body in a perfect state, by artificial means. As the Egyptian priests were the most educated, they were also the most suitable persons to be engaged in discovering means for the realization of such cherished hopes. In the meantime it was also discovered, that there are in the desert various kinds of salts, which cannot be decomposed. These salts were then used as a preparation for the corpse before it was put into the sands in the sun to dry it ; so they progressed in the art of embalming, till experience taught, that an animal body cannot be preserved for a very long time, except the entrails be entirely removed from it. Accident or continued investigation then pointed out asphaltum and balms, and the method of applying them, till at length the art of embalming was completed.

As all natural occurrences are turned into supernatural, when they are only not common, so was it also with this Egyptian art. The belief was then gradually induced, that the souls of the dead that were found in the sands were near to them, and preserved them in their former condition, unimpaired, and also uncover the sands that they might be taken back to their relatives. The soul consequently appeared to demand that the body, her former tenement, should be preserved and buried. It became then the duty of the Egyptians to obey the demands of the souls of their departed friends. The priests afterwards taught, that the soul, which was *in* the body when it lived, is *at* the body when dead but not decomposed ; but as soon as putrefaction takes place, the soul departs from it forever. To this question attached itself very naturally another one—where does the soul go, when it leaves the body altogether ? The priests answered, if the soul has not lived a virtuous life on earth, it enters the body of a new-born beast ; but if it was distinguished for a godly life, it goes to the gods. The human soul that enters the body of a beast, changes its tenement with the death of that new body, and enters into another one, till the space of three thousand years expires, and then it enters again a human body, after which it is taken to the gods.

[To be continued.]

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON, DECEMBER 8, 1852.

Law and Physic.—Those of our readers who are members of the Massachusetts Medical Society will feel an interest in the progress of the suit, Barrows *vs.* B. Carpenter *et al.* It will be recollected that the plaintiff commenced an action many months since, against two medical gentlemen, members of the society, for a libel, contained in the journal of proceedings published in the Appendix to the "Communications," laying the *ad damnum* in the modest sum of ten thousand dollars! The case came on at the late term of the Supreme Court, held at New Bedford. After the opening was made for the plaintiff, Fletcher J., on examination of the declaration in the writ, expressed a strong doubt whether the words set forth were actionable at all, and suggested that merely such evidence should be then introduced as would make a case, upon which the full bench might decide whether the action would lie. In the event of their decision being such as to sustain the declaration as libellous, the case will then be ready for a hearing before a jury, when the participation of the defendants in the publication, if any—the extent of damages—the protective rights of such associations, and the like incidental questions, will be passed upon. If the full bench decide that there has been no libel, the case, of course, drops.

J. Kearney Rodgers, M.D.—A memoir of the life of this distinguished surgeon was read before the New York Academy of Medicine, on Wednesday, Oct. 6th, by Edward Delafield, M.D., and has since been published. Dr. Rodgers seems to have been born to a position, and although the president of Nassau Hall predicted that he would never distinguish himself, the fact of being thus rebuked appears to have stimulated him to rise above the indolence that sometimes weighs down genius which has apparently no motive for action. He became a model of scientific industry through his medical pupilage; and during a long professional career, sustained a reputation of the first order as a citizen and a professional man of skill and unblemished fame.

Dr. Delafield has been particularly happy in the character he has drawn of his excellent friend. With such materials, however, it would have been extraordinary had he failed in the construction of a biographical sketch of interest and profit to the brotherhood. An elaborate literary production was not contemplated. A plain narrative of the youth and age of a great surgeon, with whom every member of the audience was probably well acquainted, was a topic full of interest to them. We are personally indebted to the author for the gratification his discourse has afforded, and the medical public will every where, it is believed, express the same sentiment of heartfelt satisfaction.

Chimpanzée Skeleton.—We have derived much gratification from reading a paper by S. Kneeland, Jr., M.D., on the skeleton of the great Chimpanzée, read by him before the Boston Society of Natural History. There is neither poetry nor perplexity discoverable in the article, which are very common faults in treating of subjects of a similar character. Dr. Kneeland aimed at a correct, unadorned anatomical description of the animal, and therefore whatever is written, will answer for all coming time, as a reference for the naturalist who may have occasion to compare his researches and descriptions with the specimens in the Boston Cabinet. If every one wrote on medicine in the same plain, straight-forward manner, there would be fewer mistakes and misapprehensions, and less respect for theoretical speculations.

Physician's Pocket Dose and Symptom Book.—A good idea—to make a book about the size of a wallet, containing symptoms of diseases, doses, and other kinds of information often needed at the bedside, when a practitioner is in doubt in small matters. Messrs. Lindsay & Blakiston have published a miniature volume, by Joseph H. Wythes, M.D., bearing the title here given. If great books are sometimes regarded as great evils, little ones may occasionally prove to be small blessings. There is nothing original in the design, or remarkable in the arrangement. With the fewest words, all that is necessary to be said of a medicine, for example, is briefly related. For young physicians, it will prove an excellent monitor; and the aged have often quite as much need of being reminded of what they have forgotten. Take it all in all, the dose-book is far from being a disagreeable dose.

Fœtal Circulation.—Very few students understand the fœtal circulation by simply reading about it. A pictorial illustration is worth a hundred elaborate, wordy, anatomical dissertations. That excellent artist, Mr. H. A. Daniels, of New York, whose skill is a familiar subject with many of the writers on surgery in that city, has executed a very useful drawing, illustrative of the relation of parts, the origin of the arteries, &c., beginning with the placenta. The figure is about the size of a newly-born infant, and every part is so exposed and accurately designated, that the whole scheme of the circulation is clearly shown. The cord, with its two arteries and one vein, proceeding from the placenta; the umbilical vein, dividing into branches to be distributed to the liver; and the ductus venosus, entering the inferior cava; the portal vein, pulmonary artery, ductus arteriosus, urachus, common iliac, hypogastric arteries, renal capsules, &c. &c., embracing a minute display of each and every essential part of the fœtal apparatus, is complete and useful. This should be particularly in the hands of students, because it teaches a department of anatomy not readily mastered in any other manner.

Epidemic Cholera.—An appendix to the report of the General Board of Health, of London, on the epidemic cholera of 1848–49, comprising a paper by James Wynne, M.D., of Baltimore, and presented to both houses of Parliament, has been received from London. Of course, it is a carefully drawn, scientific pamphlet, detailing cases, symptoms, and treatment, and accompanied by tabular statistics enough to gratify the most profound student; but we despair of finding any thing new on the subject of cholera, and we some time since resolved to read no more about it than may be necessary to keep up with the literature that belongs to it. In regard to its treatment, but little new has been advanced. No more success, we fear, attends it now, than on the first day of the development of the disease. If this assertion is questioned, we will simply refer the doubters to the last unfortunate town or city in which the Asiatic scourge left the community in one general mourning.

Pharmacology.—No. 1, of Vol. I, by the learned Dr. Tully, on Materia Medica, or Pharmacology and Therapeutics, was published last week. It reads well, and therefore will doubtless have the sustaining influence of the profession, to whom Dr. T. is no stranger. Having repeatedly made reference to the literary enterprise upon which he has embarked, it is by no means necessary to repeat the same sentiments, by comments on this first number of the proposed series.

Progress of Physiology.—Dr. Séquard's lectures before the medical gentlemen of this city, are well attended and extremely interesting. It is the only way to study the functions of a living body, to look on the organs in a state of action, and mark how nature accomplishes her operations. Vivisections are not common with us, and hence the demonstrations of the learned lecturer strike us with more force and surprise. Dr. Dowler, of New Orleans, is one of the boldest of the modern school of physiologists. He grapples with leviathans of the Mississippi, and exhibits phenomena in the interior of their huge bodies, that quite unhinge some of the most favorite doctrines of the old theorists. Life is still a mystery. Explorations on the living body have not yet brought to light its secret abiding place. But this is an age of progress, and under the researches of such men as we have above alluded to, it may yet be discovered.

The Public Health.—There is but little demand for medical services, either in Boston or its vicinity, at this particular period. For many years, the public health has not been more satisfactory. There are chronic diseases, and casualties, as usual, in a dense population, but no alarms from the approach or prevalence of epidemics, have disturbed the people of late, and the profession consequently have not been over-taxed with labor. At all points, throughout the Union, with the exception of a few of the southern cities where the yellow fever continues, the country is unusually blessed in respect to exemption from devastating maladies.

Castor Bean Epidemic.—One of the steamboats running between Cincinnati and St. Louis, on a recent trip was crowded with German emigrants. As might be expected, their appetite for fruit and vegetables, after a long sea voyage, was most voracious. At Selma, a short distance below St. Louis, the boat received some fifteen or twenty sacks of castor beans. Their appearance excited the cravings of the emigrants. Finally, curiosity and appetite triumphed; a bag was surreptitiously opened, a large pan full extracted, and a huge luncheon of soup prepared. In a short time, the passengers in the cabin and the officers of the boat were startled by the report that the cholera, in its worst form, had broken out on deck: the castor oil was doing its work. The bag lay exposed, and a large pot full was steaming hot on the table. The captain was ordered into quarantine.

Infusoria from Cancer in the Mouth.—Mr. Weeden Cooke exhibited under the microscope some infusoria which he had taken from a case of cancer of the tongue. It had been stated that these insects were to be found in cancer generally, but he had only discovered them in that disease when situated in the tongue. Did this arise from the warmth of the mouth? Some of these insects, as seen in the field of the microscope, appeared of the size and shape of bugs; others were of an eel-like shape. He brought forward the insects, as he thought they were interesting in regard to many points in the pathology of cancer. Were they, for instance, peculiar to cancer? Did they originate from the ova of insects getting into the mouth from the exterior?—*London Med. Soc. Proceedings, in Lancet.*

Professor John Bell.—This veteran writer, teacher and practitioner, having returned to Philadelphia, has resumed his pen, and announces a forthcoming work on Mineral Waters, as a companion to his great book on Baths, so universally esteemed as the best authority on that subject.

He invites communications from those who reside near any of the celebrated watering places of the United States, or having experience in their use as prophylactic or curative. Details are desired touching their accessibility, facilities, and accommodations, which those concerned will find their interest to furnish, obliging themselves rather than the author.—*N. York Medical Gazette.*

Medical Miscellany.—Cholera is making dreadful inroads upon the population of Nassau, New Providence. At the last advices, the scourge had reached Kingston, Jamaica.—An epidemic fever has broken out at Barbadoes, and in one instance swept off an entire family.—The yellow fever is somewhat abating at Martinico.—There are thirty female medical students in Philadelphia.—Smallpox has appeared in the Island of Corfu, one of the Ionian group, and of course produces great alarm and commotion.—A woman is represented to have recently dropped down dead, at Mill Creek, near Toronto, Canada, at the sight of blood from a wound.—Dr. Crosby, of Hanover, and Dr. Howard of Columbus, both professors of surgery, speak in excellent praise of our friend Dr. Piper's work on surgery.—Eruptions of the skin on small children, of a singular character, have been recognized by physicians at the North, the present autumn, that do not yield very readily to medication.

To Correspondents.—The addition of twelve extra pages to the Journal of to-day, allows space for several articles which have been deferred; but others still remain on hand, which will have an insertion as soon as room can be found for them. Dr. Collins's Address at Pittsfield has been received—also another communication on the subject of the new operation in dentistry, which, with the one alluded to last week, can only receive at present a mere acknowledgment.

Married—Dr. A. C. Webber, of Cambridge, to Miss L. C. Balcom.—Dr. John E. Hathaway, of Worcester, to Miss C. A. Gaffield.—At Defiance, Ohio, O. H. Allen, M.D., to Miss L. E. Hinckley.—At East Hartford, Conn., Dr. J. M. Morris, of Somerset, Ohio, to Miss Lucinda Galpin, of Berlin, Conn.

Died—In Portland, Me., Nov. 16th, Dr. F. F. Sargeant.

Deaths in Boston—for the week ending Saturday noon, Dec. 4th, 64.—Males, 32—females, 32. Accidental, 2—inflammation of bowels, 2—congestion of the brain, 1—cancer, 2—consumption, 14—croup, 1—dropsy, 3—dropsy in the head, 2—infantile, 3—puerperal, 1—fever, 2—typhus fever, 2—typhoid fever, 1—scarlet fever, 7—disease of heart, 2—disease of hip, 1—inflammation of lungs, 6—old age, 1—disease of the œsophagus, 1—palsy, 1—scrofula, 1—teething, 2—thrush, 1—tumor, 1—unknown, 4.

Under 5 years, 29—between 5 and 20 years, 8—between 20 and 40 years, 20—between 40 and 60 years, 10—over 60 years, 6. Americans, 30; foreigners and children of foreigners, 34. The above includes 2 deaths at the City Institutions.

The Esculapian, A popular Medical Paper for the People. To the Editor of the Boston Medical and Surg. Journal.—I have for a long time been convinced of the utility and growing necessity for the establishment of a periodical publication to be a medium of communication between the medical profession and the public. The newspaper press is entirely cut off from the uses of medical men by its almost invariable advocacy of charlatanism of every kind, and medical literature can never be adapted to the popular understanding, even if it would answer the purpose if it could. There is therefore, undeniably, force in the apology so frequently offered by those who leave their old medical advisers for a trial of some new system; viz: "how can we know the right from the wrong, without we believe what we read, and that is almost invariably in favor of the new doctrines?" When we consider the great influence which is brought into action in order to estrange the public from the medical profession, it is evident that nothing short of its vital truth could command the confidence and veneration with which it is still held by the better educated classes of society; yet notwithstanding all this, the healing Art fails in a great measure of accomplishing the good to mankind which it is capable of being made to do.

With the view of furnishing the public with the necessary information, not only for a guide to the best measures for the preservation of health, but to enable every one in the exercise of discretion, to make a choice of medical advisers from that class of physicians, who are alone entitled to the confidence of the community—I propose to establish a monthly periodical, to commence with the first of January, 1853. Its aims, in brief, will be as follows:

1st. To show why the old established system of medical practice is alone entitled to the confidence of the public.

2d. The communication of such information as will lead to the better preservation of health, embracing articles on sanitary laws, habits of life, food, dress, climatic influences, ventilation, etc. etc.

3d. Homœopathy, hydropathy, patent medicines, &c., will be treated as the subjects demand.

Having already conversed with a number of eminent medical men, who, on a full view of the subject, have given their hearty approval of the undertaking, we shall hope to receive the same encouragement from our brethren throughout the country, and also their "material aid."

It is to the members of the medical profession that we shall look for assistance in the bringing this periodical before the public; and if they will act with any thing like the unanimity which characterizes the advocates of homœopathy, or hydropathy, there need not be a town in the United States in which we shall not have the opportunity of setting forth the true principles essential to the public health, before the year is ended. We do not mean simply, in bringing our periodical before the public, but in communicating articles for publication upon the abuses which come under their observation. To favor this end, we propose to send *ten copies* for one year, to one address, for *five dollars*. Single copies, one dollar a year. Now it is hardly possible that there is a physician in the land, but can easily procure *ten* subscribers; and moreover, it is scarcely probable but that the influence of our paper will contribute more than double the price of subscription to his interest, independent of the public good which must necessarily grow out of it.

Any person who may wish to see a sample number before subscribing, may obtain one by forwarding, post paid, *three* letter stamps to

C. D. Griswo[illegible]d, M.D. 108 *Nassau street, New York.*

On a New Mode of Operating in Varicose Aneurism. By M. Malgaigne.—The difficulty which usually occurs in operating for this, in securing the two ends of the artery, while the veins are incessantly pouring out blood, induced M. Malgaigne to try a new plan of procedure, by which opening the sac, or the integuments covering it, might be avoided. The case was an aneurism resulting from venesection, performed ten or twelve weeks before. A small pulsatory tumor existed at the bend of the arm, which caused little inconvenience. The artery was taken up, by means of two separate incisions, just below and just above the tumor, and the cure was rapid and complete, so that when the patient was seen 7 months afterwards, no traces of the aneurism could be discovered.—*Rev. Med. Chir.*, tom. xi. p. 155.